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ADAPTATION AND THE PROBLEM OF "ORGANIC PURPOSEFULNESS"¹

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I. THE REALITY OF THE PROBLEM

DESPITE the "revolutions of thought," which succeed one another with rather bewildering rapidity these days, we may occasionally listen with profit to the voice of a past generation. And I can not believe that we are yet in a position to wholly reject Herbert Spencer's well-known characterization of life as a "continuous adjustment of internal relations to external relations." Now it is this process of adjustment to which we give the name adaptation, and the special structures or functions by which the adjustments are carried out are called adaptations.

By earlier biologists and philosophers these facts of adaptation and adaptedness were regarded as among the most fundamental phenomena of life. It was facts such as these that furnished ammunition for Paley and a whole succession of natural theologians. It was these which Lamarck sought to explain by his theory of evolution through functional activity, and which Darwin attributed to the action of natural selection. And it is in this same realm of facts that the vitalists find their rea-

¹ In its main outlines, this paper was written about five years ago. It was submitted for publication February, 1918, and since then has undergone relatively little revision. For this reason, adequate reference has not been made to certain recent papers.

sons for attempting to remove biology from its place among the natural sciences.

Now it is a curious circumstance that recent developments of the evolution theory have carried us continually farther from an explanation of adaptation. Natural selection, in the Darwinian sense, has been relegated to a secondary position, while the Lamarekian principle is denied *in toto* by many. On the other hand, all that the mutationist can tell us in regard to the matter is that such useful characters as spring full-fledged into existence are not likely to be eliminated. Thereupon, the vitalist takes fresh hope and asserts the inadequacy of what he calls "mechanistic" biology to account for progressive evolution.

Of course, one way to solve a problem is to deny that the problem exists. And this is what is being done by various persons who are interested in minimizing the difference between the living and the non-living. Thus one physiological botanist, Livingston,^{1a} tells us that anything organic or inorganic is adapted to do just those things which in reality it is found to do. And he seems to think it quite as reasonable to speak of the adaptation of fragments of pumice to float on water, as of the adaptation of a flower to insure the visits of insects. The "concept of purposeful adaptation," which still plays such an extensive rôle in biology, is due to the fact that "ours is a developmentally young science," retaining "features of its early youth." Sooner or later this concept will be "totally abandoned, even as the same concept has already been abandoned by the other natural sciences."

Now it may well be that a more mature state of science will enable us to dispense with such naïve expressions as imply that an organ has a function to perform in the economy of an animal. And it may be that growing enlightenment will lead us to replace such a primitive notion as that of life by chemical affinities, electric charges and what not. But until that happy (or unhappy!) day

^{1a} AMERICAN NATURALIST, January, 1913.

arrives, I think that most biologists will continue to regard the origin of adaptive characters in animals and plants as offering a real and important problem for solution.

Again, Parker,² while going to no such lengths as this in denying the significance of organic adaptation, expresses his belief that

the majority of animal reactions are, in all probability, neither conspicuously advantageous nor disadvantageous to the life of the individual. They are dependent chiefly on the material composition of the given organism, and, so long as they are relatively indifferent to the continuance of life, they pass without special consequence. . . . The world at large affords an environment in which each animal has a wide range for possible reactions, and of a number of responses that might be made to a given set of conditions, one may be quite as appropriate for the continuance of life as another. In other words, versatility seems to be a more truthful description of actual conditions in animal life than the rather rigid state implied in the idea of adaptive responses.

It may be freely granted that much ingenuity has been displayed in discovering adaptations which probably do not exist. And it is doubtless true that many organisms may live indifferently under a wide range of conditions, or may eat indifferently a wide range of foods. But is not this condition of versatility itself a fact of adaptation? The "continued adjustment of internal relations to external relations" implies that the external relations change. Man, it is true, may live indifferently on the equator or within the Arctic circle. But would any one maintain that the physiological states which adapted him to these unlike conditions did not differ widely in the two localities? I may make a meal equally well of meat or of vegetables. But the digestive fluids secreted for the occasion would differ in the two cases.

Again, because two wholly unlike plants grow side by side in the same soil, it does not follow that they are adjusted in quite diverse ways to the *same set of conditions*. The environment of an organism doubtless comprises the totality of things which surround it. But the *effective* environment comprises only those things with which

² AMERICAN NATURALIST, January, 1913.

the organism comes into functional relation. And as the organism evolves, these effective elements become different.

As I see the situation at present, the fact of organic adaptation remains the central one in evolution, and indeed the central one in biology. I shall give no further time, therefore, to justifying a rather laborious attempt to show how this fact may be accounted for without carrying us outside the limits of natural science. Before passing on to this discussion, I will merely remark that I place in the category of adaptation anything which increases the adjustment of the organism to the conditions of its existence, whether or not this may ever have a determining influence in the preservation of life. Many such adjustments have arisen in our own race which certainly can have played no part in the survival of the individual or the race. An example of this is the case dwelt upon by Spencer, of the correspondence between the nicety of tactile discrimination on various parts of our skin and the relative frequency of contact with foreign objects on these surfaces. And such cases could be multiplied indefinitely. Nevertheless, much recent biological speculation has been vitiated by the identification of adaptation with self-preservation.

II. ADAPTATION AND "CONTINGENCY"

If an intelligent animal is confronted with the necessity of taking action to avoid injury or secure food, two ways only would seem to be open to it:

1. It may consciously adapt its actions to this end, or
2. It may go through a series of more or less random movements until it happens to make one which is fitted to the needs of the situation.

On first thought, it might seem that these two modes of procedure were radically distinct, and indeed, in a sense they are. Considered historically, however, the second may be regarded as a step in the development of the first, or, to express the same thought otherwise, intelligent action is in every case the outcome of earlier experi-

mentation. We can foresee the results of an action, only in so far as they have been experienced before, either in a situation identical with the present one, or at least in situations having certain elements in common with it. Furthermore, in the early life of the individual, the movements of the so-called "voluntary" muscles were in a high degree random and undirected. The association between a given muscular contraction and a given result in consciousness must, in the first instance, have been purely arbitrary, and could not have been anticipated prior to experience.

Thus to restate somewhat paradoxically our original proposition, an intelligent animal attains a sought-for end, either by blundering into it or by directing its course on the basis of past blunders. In either case, the association between the means employed and the end attained is, in the last resort, accidental. At the outset, the idea of the end did not in any direct way call forth the means to its realization, however purposive the action may appear when fully perfected.

Let us extend our argument to those fields of organic activity from which intelligence seems to be largely or wholly excluded. In instinctive actions, even more than in intelligent ones, a series of movements proceeds unflinching to a given end, as if directed by the latter. In earlier days the adaptive instincts of certain lower animals furnished some of the most telling arguments for the special interposition of an all-wise Providence. Today, as biologists, we commonly explain these movements on the basis of an inherited "mechanism." We may believe, with Loeb and others, that we have to do with a chain of reflexes, each serving as a stimulus to call forth its successor at the appropriate moment.

How this mechanism arose is a disputed point, but there are two principal hypotheses as to its origin: (1) Instinctive actions are ones which originated, intelligently or otherwise, in the course of individual experience, and finally became fixed through heredity; and

(2) they are the result of natural selection, acting on congenital tendencies toward such movements as proved to be adaptive.

Without discussing the merits of these rival theories, which are by no means mutually exclusive, I merely wish to point out that both of them assume a complete *contingency* as regards the relation of means to end. On the assumption that instinct is inherited habit, the actions, before becoming habitual, must have been performed either intelligently or as a result of blind groping. In either case, their adaptedness to the end in view was, at the outset, accidental, as we have already seen. On the assumption that instincts have arisen through natural selection, chance tendencies toward movements of an adaptive sort were perpetuated. Here, the complete contingency is obvious, unless we assume some directing influence determining the nature of the variations. I shall return to this last point later.

Still lower than instincts, in the scale of organic behavior, we have the various responses to stimuli which are known as "tropisms" or "taxes." Under this head are included the locomotion of the organism to or from a source of stimulation, or, in the case of a fixed organism, the assumption of a definite position, or the arrangement of its parts, with relation to the direction of the stimulus.

Here, again, we have two rival hypotheses, which are not, it seems to me, wholly antagonistic. According to one view, the organisms are "fatally" turned to or from the source of light, heat, or the like by the unequal stimulation of the opposite sides of the body. When the appropriate orientation has been brought about, the two sides of the organism are equally affected, and further locomotion will be in line with the source of stimulation.

The other view lays stress on those cases in which organisms are not drawn directly towards or away from a stimulus, but undergo random movements, having no primary relation to it. In what are regarded as the most primitive cases the stimulus which results in a change

of behavior is usually a noxious one, leading to a backing out or turning aside. When forward movement is resumed, it is a matter of chance whether the organism remains in favorable surroundings or finds its way back to the unfavorable ones. If the latter, the "avoiding reaction" recurs, and the performance is repeated until it leads to a more fortunate issue. The invariable "pull" or "push" of the tropism theory is not regarded as the primary phenomenon, though an observer who viewed only the end results of the process might easily believe that they had been brought about by such a directing influence.

Here, again, it is not my purpose to discuss the merits of these rival hypotheses. It is possible, indeed, that they should be regarded as complementary, rather than antagonistic. Jennings admits that responses which originally were performed according to the method of "trial and error" may, through the abbreviating influence of habit, come to be determined more directly by the stimulus. But, however we may view the method of origin of these responses to stimuli, it seems plain that any adaptiveness that we meet with is contingent in the sense in which I have already used the term. According to the investigations of Jennings, the organism reaches an optimum environment by chance, and remains there because it is stimulated to change its course whenever it begins to pass out of this environment. That unfavorable stimuli should provoke these changes of behavior need not be attributed to any "primary purposefulness" in living matter, since we can be perfectly sure that any organisms behaving differently would be speedily eliminated.

Again, I take it that the chief advocate of the theory of direct orientation would be the last to assume a principle of primary adaptedness, and would admit that any utility connected with these "tropisms" must have been, in the first instance, a pure coincidence. In the case of an organism, "irresistibly" drawn toward a favorable

stimulus, as a growing plant toward the light, we might seem to have an instance of such a directly purposive action, *i. e.*, the determination of the means by the end. But several things must here be taken into consideration. (1) It not infrequently happens that organisms are drawn in an equally irresistible manner toward a fatal stimulus, *e. g.*, the moth to the flame; (2) we can not feel sure, in every case, that the attainment of the goal is not the outcome of random movements, unperceived by the observer; (3) even where the response is indubitably adaptive, and as direct and unfailing as a simple reflex, it may be the outcome of a mechanism developed through natural selection, *i. e.*, the survival of random variations which were as frequently unadaptive as they were adaptive. The fact that some organisms still make suicidal responses to less familiar stimuli favors this last view.

Next, we may consider the phenomena of metabolism, growth and development. We group these things together, because they can hardly be considered separately. Growth is the outcome of metabolism, and development of metabolism and growth.

The phenomena revealed through studies of normal physiology and embryology are obviously highly "purposive," in the sense that they have relation to the attainment of an end, that end being the preservation of the individual and the race. Nevertheless, they are believed by most biologists to be the outcome of a "mechanism" the functioning of which presents no greater difficulties, apart from complexity, than the working of a clock or a steam-engine.

When we come to consider the origin of this mechanism, we may mention three chief hypotheses, which have been or still are held. (1) It may have been specially created by a super-mundane power in each individual species of organism; (2) it may have gradually developed out of simple beginnings by the "selection" or survival of random variations which were as likely to be unadaptive as adaptive; or (3) it may have gradually

developed out of simple beginnings through (a) direct responses to environmental stimuli, or (b) the effects of functioning upon the functioning parts themselves.

The first of these alternatives has been well-nigh discarded by scientist and layman alike, and need not be further considered here. I will point out in passing, however, that certain elements of the "special creation" hypothesis have recently been put forward in the name of science. Of this more anon.

The second alternative, that of natural selection, is admitted by most biologists to be one of the factors concerned in the production of adaptive mechanisms, though it is doubtful whether any two thinkers would agree as to the importance to be assigned to it. The essence of this hypothesis is the contingency of the individual variations in relation to the need to be satisfied. If the variations are *directed*, in the sense of tending preponderatingly toward the satisfaction of this need, then our explanation is shifted to a totally new basis. It is this directive tendency, not natural selection, which is the effective agency in evolution. The consequences which would follow such an assumption will be discussed later.

The third of our alternative hypotheses has figured historically as the chief rival of natural selection, though by many (*e. g.*, by Darwin himself), both principles were accepted. One of the merits of the Lamarckian principle, in the eyes of some of its adherents,³ is its apparent rejection of contingency or chance, a fatal weakness, so they believe, in the natural selection theory. But a little thought will show us that the Lamarckian principle, no less than the Darwinian, is based upon chance, as regards the relation between the need and the means to its fulfillment.^{3a}

³ August Pauly, "Darwinismus und Lamarckismus," 1905; R. H. Francé, "Der heutige Stand der Darwin'schen Fragen," 1907.

^{3a} It is true that both of the writers cited in the preceding footnote clearly recognize this *accidental* character of adaptive responses in their inception, though failing to realize the significance of this fact for biological philosophy. (See my review of Pauly, in *Journal of Philosophy, Psychology and Scientific Methods*, August 27, 1908.)

Let us consider (*a*) the case of modification through direct environmental stimuli. There is much vague talk about the "environmental mould," in which the "plastic" organism is supposed to be "cast"; but those who have given much study to the subject recognize that modifications produced by the environment are in the nature of reactions to stimuli. In many cases, these reactions are plainly adaptive, in the sense of furthering the life or comfort of the individual or the race, as when a callosity is developed in consequence of continued friction, or an antitoxin is generated to combat a bacterial poison. The fact, however, that there are varying degrees in the adaptiveness of these responses, and indeed that many of them appear to be wholly unadaptive, suggests the probability that the truly adaptive ones, when at all constant, have resulted from the selection of "accidental" variations. This can, of course, be true only of responses to environmental stimuli which have presented themselves frequently in the history of the race. Cases in which the organism has responded adaptively to stimuli quite new to racial experience are not, however, entirely unknown. These will be discussed in a later section. It may be said in passing, however, that the only conceivable scientific explanation of such cases involves the principle of "trial and error," which, of course, is based upon complete contingency as regards the relation of means and end.

Let us pass to (*b*) the effects of functioning upon the functioning parts themselves. It is held by the Lamarckians that organs or parts grow or diminish through use and disuse, and that the perfected mechanisms which now arouse our admiration and wonder are the outcome of past functional activity. Many upholders of this view introduce the idea of a conscious struggle toward a desired end. The perfecting of the parts they regard as a voluntary process. Thus is "blind chance" cast out as a factor in evolution. But such reasoning rests on insufficient analysis. Granting the part played by voluntary action (*e. g.*, exercise or practice) in the post-

natal development of many higher organisms, we need only refer the reader to what we have already said about the "contingent" character of even intelligent action. But it seems likely that the claims of the psycho-vitalists (*e. g.*, Pauly and Francé) are largely fantastic, and that voluntary struggle toward an end has not played the important rôle in organogenesis which they imagine that it has. The greater part of the functioning of the organism probably consists in blind responses to external or internal stimuli—blind in the sense of having no conscious end in view. Thus regarded, they are in no way different from the responses already considered under (*a*), save that we there dealt with the effects of external stimuli alone.

Accordingly, we may repeat here that so far as these functional responses—and the organs they perfect—are adaptive, their adaptiveness must have arisen, in the first instance, by the selection of contingent variations. Under this head are to be included (1) the preservation of those individuals which chanced to make appropriate responses (natural selection); and (2) the making habitual on successive generations of individuals of responses which chanced to fulfil a given need when first experienced (Lamarckism). The only other alternative would seem to be some sort of inscrutable foreknowledge on the part of the organism of every need to be experienced, and of the way in which this need could be satisfied. Such a conception would obviously carry us beyond the field of scientific explanation, but I shall none the less consider it in its proper place.

It is not my purpose here to discuss the arguments for or against either the Darwinian or the Lamarckian principle. It is my object merely to point out that *both theories rest on the selection, in one way or another, of variations which were originally contingent or accidental, in the sense of not being directly determined by the need to be fulfilled.* And, indeed, this is true of all of the other rival or subsidiary hypotheses of evolution,

so far as they may be regarded as scientific theories at all.

The theory of mutation, in its original form, postulated large and abrupt variations as the material for selection, a modification which does not affect the principle essentially. In its later form, it merely insists that these variations must be of the discontinuous or Mendelian type, assuming that all other variations are non-inheritable. Those who maintain the importance of isolation in evolution can not, of course, regard this as a *vera causa* of adaptive change. The actual changes must be either "spontaneous" variations or mutations or else modifications due to environment. Thus, we must resort finally to either the Lamarckian or the Darwinian principle to account for such of them as prove to be useful. "Orthogenesis," so far as it is not a vague appeal to a "perfecting principle," "élan vital" or the like, is a mere assertion that variations may accumulate in a given direction independently of selection. Wherever the variations are sufficiently adaptive, however, we are not justified in excluding selection. When non-adaptive, such a process presents no greater difficulty in principle than the continuous growth of a crystal or the continuous deepening of a canyon by erosion.

Most of us are prepared to admit that much in the organic world is non-adaptive. We may even grant that a large proportion of the diagnostic characters of species and genera belong to this category. Such characters, while they may baffle the investigator, are in general not such as would have suggested the operation of a supernatural factor in evolution. In this paper we are concerned with the problem of organic adaptation, and shall leave aside the origin of characters which are useless to the organism.

In the foregoing analysis, I have regarded adaptive response, whether of structure or function, as being invariably a secondary phenomenon. The connection between the need of the organism and the means adequate to satisfy this is believed to have always been, at the out-

set, an "accidental" one. In those cases where the correct response appears to ensue unhesitatingly, we have had to suppose either (1) that the observer has overlooked "trial and error" stages preceding the response in question, or (2) that the response is the outcome of an inherited mechanism, based upon racial experience, and therefore ultimately upon some form of selection.

III. VITALISM⁴

Let us now consider the claims of a school of thinkers who argue for the existence of a primary purposefulness in living things, and who deny that any conceivable mechanism can account for certain of the phenomena observed. As the most conspicuous representative of this school we naturally turn to Hans Driesch, who has made a more determined attempt than any other vitalist to reduce his beliefs to a unified system of philosophy.

Driesch's three "proofs" of vitalism may be summarized as follows:

1. In the earlier development of some organisms, rather low in the scale of life, any part of the embryo, provided that it be of a sufficient size, will, if artificially detached, produce the entire organism. This he regards as conclusive disproof of the supposition that the spatially arranged diversities of the adult organism depend for their origin upon diversities of a *spatial* sort in the embryo. Such a spatial prearrangement of the parts as is postulated by the Weismannian "germ plasm" theory, and other preformationist hypotheses, he assumes to be essential to any mechanical theory whatever.

But, Driesch claims, the spatial diversities of the adult organism must depend upon preexisting diversities of *some* sort, therefore he invokes a non-spatial agent, "entelechy," to account for them. Now "entelechy" must be a manifoldness, since it is conjured up to explain other manifoldness, but this manifoldness is *intensive*,

⁴ In this section, I have made free use of a review of Driesch's "Science and Philosophy of the Organisms," which I wrote some years ago (*Journal of Philosophy, Psychology and Scientific Methods*, June 9, 1910). I have not thought it necessary, however, to indicate the extent of these quotations.

not extensive. As an illustration of an "intensive manifoldness" he instances one of our own states of consciousness, in which many elements are presented simultaneously, though not spatially separated from one another. But entelechy is not to be identified with mind. It is an unknown something which stands in the same relation to our mental life as it does to other organic phenomena.

2. Driesch's next "proof" of vitalism is somewhat similar to the first, though it rests upon the facts of normal life history, instead of upon artificial disturbances of this. The primitive germ cells, each of which, according to the hypothesis he combats, should contain the "machine" or spatial prearrangement of parts necessary for the development of an entire organism, undergo in the gonads an extensive series of divisions, leading to the formation of the mature ova and spermatozoa. "Can you imagine," he asks, "a very complicated machine, differing in the three dimensions of space, to be divided hundreds of times and in spite of that to remain always the same whole?"⁵

3. The last "proof" of vitalism is based upon an analysis of animal behavior. Driesch makes much of the fact that an action of a higher animal, particularly of an intelligent one, is something more than the sum of many simpler elements, each depending upon an element in the complex of stimuli to which the organism responds in a given case. The response of the organism is a unified whole, corresponding to a total situation in the outer world. A slight change in this complex of physical stimuli, provided that it has *significance* for the organism, may result in a totally different kind of response. On the other hand, an entirely different set of physical elements—having, however, the same *meaning* for the organism—may call forth precisely the same response. In other words, there is no functionality (in the mathematical sense) between the response and the stimulus.

This line of argument, different as it may seem, rests

⁵ "Science and Philosophy of the Organism," Vol. I, p. 225.

upon the same fundamental assumption as the two preceding ones, namely, that a truly mechanical theory must find in the cause as many separate elements as we observe in the effect. The structural diversities of the adult organism must rest upon corresponding structural diversities, present from the beginning in the germ. The functional diversities, constituting a complex act of behavior must rest upon corresponding functional diversities in the stimuli which make up the total effective situation. If no such correspondence can be shown, we must invoke some principle of a totally different nature from those which we employ as explanations in the inorganic world.

Now, such a conclusion as this seems to rest upon an insufficient consideration of what really happens in the inorganic world. In a sense, the solar system was present potentially in the original homogeneous nebula, while the various continents and oceans, mountains, lakes and rivers of the world we live in were all present potentially in the molten globe which in some way detached itself from the parent mass. But there was certainly no "preformation" of these final products of cosmic evolution. The diversity which was introduced was totally new. In the language of biology the world's development was strictly "epigenetic." And yet the process was none the less mechanical, as every vitalist will allow. Why then does Driesch insist that a mechanism adequate to account for an animal's ontogeny must present a part-for-part correspondence with the adult organism? For it is only a mechanism, *as thus conceived*, that is disposed of by his "proofs" of vitalism. His experiments compel him to dismiss the notion of a *spatial* prearrangement of parts. Therefore, he jumps to the conclusion that there must be a *non-spatial* prearrangement of parts—an "intensive manifoldness." But why should there be any prearrangement of parts at all? Is it not a fallacious philosophy which insists on such an exact numerical cor-

respondence between the elements of the cause and the elements of the effect?⁶

Despite these logical difficulties, Driesch's third "proof" of vitalism contains such an unmistakable element of plausibility that some further consideration may profitably be given to it here. His contention is summed up in the phrase "individuality of correspondence" between stimulus and reaction. "It is not the single constituents of the stimulus," he says, "on which the single constituents of the effect depend, but one whole depends on the other whole, both 'wholes' being conceivable in a logical sense exclusively" (II, 81). Why is it that we react to *objects* rather than to sensuous images? "The dog, 'this dog,' 'my dog,'" to quote Driesch, "is 'the same' stimulus, seen from any side or at any angle whatever: it always is recognized as 'the same,' though the actual retinal image differs in every case" (II, 73). Experience and association, he thinks, afford an insufficient basis of explanation here. There must be something capable of resolving past experience into its elements and making wholly new combinations of them.

Driesch challenges his opponents even to conceive of a machine that could accomplish results such as these. This introduction of the word "machine" would seem to prejudice the case in his favor at once. But is he not really challenging us to imagine how phenomena that require sense organs and a nervous system for their performance could be performed by some other type of mechanism which is simpler and more fully understood by us. Confessedly we can not do so. Looking at the subject in an unbiased way, it would seem that the nervous system had the appearance of a finely wrought mechanism to a higher degree than any other portion of the body. It is truly one of almost infinite complexity, and one that is largely inaccessible to experimental observation. But certain significant facts have been demon-

⁶ Spaulding (*Philosophical Review*, July, 1909) and Jennings (*Johns Hopkins University Circular*, No. 10, 1914) have already called attention to the fallacy of this aspect of Driesch's argument.

strated none the less. Sherrington has described in some degree the mechanism of inhibition, and has ascertained some of the factors which determine which of two simultaneous stimuli shall prove effective in a given reflex. Do not such data at least help us to conceive the possibility of a nervous system whose activities may be understood without the aid of an entelechy to make its decisions for it?

The emancipation of the organism from the controlling influence of immediate stimuli is admitted to be one of the salient features in animal evolution. Now, in order that present activities may be directed with reference to future results, the stimuli must become more and more symbolic, *i. e.*, they must acquire a "meaning." That one thing may "stand for" something else, and call up the responses proper to that something else may readily be understood in terms of association. At least there would seem to be no desperate need for invoking "entelechy" at this point. If this be granted, why should we expect any correspondence between the sensuous elements of the stimulus and the elements of the response? The effects of a given "individualized stimulus" are dependent rather upon the aggregate of associative processes which this stimulus calls up. And this aggregate is altogether an empirical one, not a logical one as Driesch supposes. The connections that bind it together may be quite arbitrary and accidental. It is partly the product of individual experience, partly of racial experience—this last on any theory of inheritance. That several widely different stimuli, having the same meaning (*i. e.*, having certain important associations in common), can bring about essentially the same response would seem, on the face of it, no more difficult to understand "mechanically" than that several very differently shaped keys can open the same lock.

The weakness of Driesch's "third proof of vitalism" would seem, therefore, to be twofold. (1) He appears to believe that an explanation, in order to be mechanical,

must find a definite correspondence between separate factors of the cause and separate factors of the effect,⁷ and (2) he appears to believe that in any mechanical explanation of action the character of the response must be determined by the immediate sensuous stimuli themselves, without regard to the representative (associational) character of these stimuli.

Driesch, like other vitalists, lays great stress upon "adaptive" or "regulative" phenomena, though he makes no claim that these necessarily demonstrate the truth of vitalism. Indeed, it will be noted that the three foregoing "proofs" rest on quite other grounds. We may safely say, however, that for most biologists the great stumbling-block to a consistent mechanical explanation has been this central fact of organic "purposefulness." In pre-Darwinian days the whole subject was a mystery, which science cheerfully handed over to theology for solution. Later, we grew accustomed to the idea that much which seemed purposeful in nature was the outcome of "chance." But for many there was always a considerable residuum which defied solution. For there certainly seem to be cases of adaptive response to wholly new situations, that can not be accounted for on the basis of an evolved mechanism. And furthermore, it is now obvious that no single theory of evolution yet proposed, nor, indeed, all of them combined, can adequately account for much that has come to pass.

In the face of these perplexities, it is but natural that many have taken refuge once more in various intangible forces and principles, almost wholly devoid of positive attributes, and agreeing only in their alleged competence

⁷ It must be admitted that explanations of this type have been put forward by avowed mechanists. Thus Loeb ("Mechanistic Conception of Life," p. 80), in discussing the present writer's experiments upon the color changes of flatfishes, concludes that there is an actual reproduction on the skin, through the brain, of the retinal images of the background. I think that a careful reading of my own discussion of these experiments sufficiently disposes of this contention (*Journal of Experimental Zoology*, May, 1911). The yet more extensive experiments of Mast (*Bulletin of the Bureau of Fisheries*, Vol. XXXIV, are likewise conclusive against this view.

to "explain" otherwise inexplicable facts. Of these Driesch's "entelechy" and Bergson's "élan vital" are but types.

Much less discordant with our scientific habits of thought are the utterances of some of the so-called "psycho-vitalists," to whom allusion has already been made. These writers do not have recourse to metaphysical principles, wholly beyond the realm of experience. They invoke the familiar facts of conscious purpose, intelligence and will. Organic happenings *seem* purposive, they think, because they *are* purposive, in the same sense that our own voluntary actions are purposive. Such a view carries the realm of mental life far beyond the bounds which we are wont to assign to it. Its logical outcome is a thoroughgoing panpsychism, an outcome which some of its advocates are quite ready to accept.

Now, it seems to the writer that a panpsychic view of nature can be stated in such terms as not only to be plausible, but to meet certain of our most fundamental intellectual needs. But such a view is at best a philosophical creed, not a scientific explanation, and should never be offered as a substitute for the latter.

The introduction of will, purpose, etc., in the rôle of scientific explanations may have one of two implications. Either (1) it may be assumed that a given physical configuration, plus these psychical concomitants, is able to accomplish what would be impossible for the *same* physical configuration minus these psychical concomitants (interactionism); or (2) it may be assumed that only that type of physical configuration which is invariably bound up with certain psychical factors is competent to call forth the result in question (parallelism). According to the second point of view, the question whether the same result would have ensued without the agency of purpose or will is an absurdity. If purpose and will had been lacking, the physical antecedents would of necessity have also been different.

It is needless to say that both of the foregoing positions have been upheld by philosophers. It is my wish

to point out, however, that on neither assumption does the introduction of conscious purpose supply a missing link in our explanation of the "teleological" in nature. Whether or not we admit the efficacy of mental states, independently of their physical concomitants, we have already seen that conscious purpose must proceed on the basis of experimentation. It must have learned through trial that a given means will lead to the attainment of a given end. The existence of any primary foreknowledge of the relation of means to end is contradicted in our own every-day experience.

It may be useful to introduce a description by a psychologist⁸ of what actually occurs when we are trying to solve a problem:

Our only command over it is by the effort we make to keep the painful unfilled gap in consciousness. . . . Two circumstances are important to notice: the first is, that volition has no power of calling up images, but only of rejecting and selecting from those offered by spontaneous redintegration [=association]. But the rapidity with which this selection is made, owing to the familiarity of the ways in which spontaneous redintegration runs, gives the process of reasoning the appearance of evoking images that are foreseen to be conformable to the purpose. There is no seeing them before they are offered; there is no summoning them before they are seen. The other circumstance is, that every kind of reasoning is nothing, in its simplest form, but attention.

It is, therefore, a false theory of our own purposeful actions that is projected backward into organic nature by the psycho-vitalists. The existence of instinctive acts, which fit means to ends, prior to experience, in no way invalidates what I have said. For these may be assumed to be based, in some way, on past racial experience. And, in any case, so far as an action is instinctive, it can not be *consciously purposive*. Assuming that instinctive actions are performed consciously, at all, which some would perhaps deny, it is not likely that anything beyond the next succeeding step in the series is at any moment present to consciousness. The biological meaning of the entire performance (say the building of

⁸ Hodgson, quoted by James ("Principles of Psychology," Vol. I, p. 589).

a nest) can not be understood by the organism. Each step is desired and willed on its own account alone. The end *takes care of itself*, by virtue of a preestablished mechanism.

Thus purpose, in the psychological sense of the word, can not be predicated of a complex instinctive act, even though the individual steps be consciously performed. Least of all can it be predicated of a process of organic regulation or reparation, the object of which can never be consciously in view. It was doubtless in part considerations like these which led Driesch to deny the mental nature of "entelechy" altogether and to remove it to a transcendental sphere in which it was no longer subject to the exacting demands of experienced reality. Indeed, he tells us that "there *must* be a something in them [morphogenetic, adaptive and instinctive entelechies] that has an analogy not to knowing and willing in general . . . *but to the willing of specific unexperienced realities*, and to knowing the specific means of attaining them" (II, p. 142). We think more favorably of Driesch's good sense when he admits that the position of his doctrine is at this point "rather desperate." Nor is Bergson's case a bit better when he naively attempts to clear up certain of the most baffling phenomena of instinct by invoking the aid of "intuition" or "sympathy." The psycho-vitalists introduce an agent which is to some degree intelligible, even though it is inadequate. The agents which Driesch and Bergson conjure up are neither adequate nor intelligible.

In the writings of these and some other vitalists the "vital principle," by whatever name called, is distinctly credited with powers which we should ordinarily term clairvoyant. Indeed, we are forced to conclude that it must be able to "tap" sources of information which are closed even to the highest finite intelligence. This, of course, is mysticism pure and simple, though such a reproach admittedly does not constitute its refutation for all minds.

² "Creative Evolution" (trans.), pp. 173-175.

It may be interesting perhaps to consider where such assumptions would lead us. Suppose that we adopt the absolutist idea of an Infinite Knower, having cognizance of the future as well as the past, or rather including both future and past in one eternal present. By getting into connection with this, our entelechy could doubtless solve any problem which confronted it. But how, on such an assumption, could we account for the multitudinous *mis*-adaptations which confront us? How should we explain an instinct which led to the harboring of baneful parasites in an ant community or a regenerative process which resulted in the formation of the wrong organ? Perhaps these perplexing cases would be merged into the general mystery of the origin of evil, and there, indeed, may be where they belong.

But we are not compelled to accept an absolutist interpretation of things. As scientists, we may find it more easy to believe in the evolution of God, in a "*Dieu qui se fait*."¹⁰ Well and good, but then the essence of this view is the *newness* of everything that happens. No mind, however infinite, could foresee the future, for the simple reason that the future is not determined until it comes to pass. Even our deity must learn by experience, and "entelechy" would have to do the same. In that case neither would be of much service in attempting to explain organic purposefulness. Had we previously learned to expect any great amount of consistency among the various views of M. Bergson, it would have been a source of surprise to us to find him coupling together this idea of "creative evolution" with a transcendental "*élan vital*," which provides the organism with useful structures without the guidance of experience.

Such a departure as I have made from the field of legitimate scientific discussion may shock those of my readers who shy at anything suggestive of metaphysics or theology. But we have been told with increasing frequency of late that our accepted scientific methods

¹⁰ I believe that this expression is Bergson's.

have broken down in the face of vital phenomena, and that the only path of escape was one which logically led to mysticism. For this reason it seemed worth while to inquire whether even this abandonment of our scientific principles would lighten our difficulties.

Now, while we believe the solutions offered by the vitalists to be but pseudo-solutions, we must admit that the issues they have raised are real ones. It is to the great credit of this school, and of Driesch in particular, that they have awakened some of us biologists from our "dogmatic slumber" and forced these problems upon our attention. The problems are real ones in the pragmatic sense of determining our attitude, both theoretical and practical, toward biological investigation in general. Most important of all, vitalism has unearthed a number of highly interesting experimental data, which it challenges its opponents to explain. To this extent it may lay claim to the rank of a "working hypothesis."

Let us consider some of the points at issue between vitalism and what I shall call "scientific biology." In what follows, I have stated what I believe to be the typical attitude of each side, though it is likely that no two persons would agree in every particular.

1. Scientific biology is strictly deterministic. It admits the possibility of only one result from a given set of antecedents. Vitalism is indeterministic, holding that from precisely the same antecedent situation more than one result is possible. Driesch saves the principle of "univocal determination" by saying that in cases where different results follow the same physical causes, there must have been a difference in "entelechy." But Johnstone,¹¹ a disciple of Driesch, throws over even this formal adherence to scientific method, and asserts boldly that there must be "uncaused differences" in the organic world. He illustrates this belief from the variability among the millions of eggs spawned by a single flounder. The usual explanation, based upon differences in exter-

¹¹ "The Philosophy of Biology," 1914.

nal conditions or on imperfections in the mechanisms of cell-division he holds to be inadequate. Now, in his belief, it is these "spontaneous" variations (using the former word literally) that furnish the raw material for evolution.

Jennings¹² sees in this postulate of indeterminism the fundamental fallacy of vitalism. It certainly is the feature that would most seriously affect us as investigators. For whether variations are regarded as uncaused or as caused by an agent beyond the ken of scientific investigation matters little. Any attempt to account for them by experimental or observational means must be futile.

2. Scientific biology endeavors to explain organic phenomena on the basis of antecedent physical conditions, though admitting that our knowledge of cause and effect is in the last resort empirical, to the extent that much which happens could not have been predicted in advance. Vitalism explains organic phenomena—or a certain part of them—on the basis of ends to be realized, and gives to these ends a determining influence in providing the means to their realization. Since the *a tergo* "push" of physical causation would only by rare chance be directed in harmony with these ends, vitalism introduces a non-physical agent to guide or control the former. Driesch goes to great lengths to explain how "entelechy" can play this rôle without coming into conflict with the law of the conservation of energy.

In a certain sense the existence of such "ends" must be admitted by all biologists. Attainment of the typical form, self-preservation, racial preservation, etc., are "ends" in the sense that organic processes in general are observed to trend in those directions. Furthermore, disturbances of this normal trend often seem to be corrected automatically. Phenomena strictly analogous in this respect can, of course, be instanced from the inorganic world. All we have to do is to designate the observed goal of such a process as the "end" and the

¹² *Science*, June 16, 1911; October 4, 1912.

causally determined steps become the means to its realization. The difference between such a physical process and a vital one, as conceived by Driesch, is that in the latter a given sequence of events *may or may not* come to pass, depending on the whim of "entelechy." The issue here, then, is practically the same as that first raised, namely, that of determinism versus indeterminism.

3. Scientific biology declares that vital phenomena are chemico-physical, in the sense that they are the inevitable outcome of the particular material aggregations which we term organisms.¹³ It grants that in these manifold chemical syntheses entirely new properties have emerged, though insisting that the same may be said of any union of elements whatever. Vitalism denies that any possible configuration of material particles, without the aid of an immaterial principle, can account for the phenomena observed. It is for this reason that "vitalism" is commonly set in opposition to "mechanism." Driesch's three "proofs" of vitalism are concerned with this last aspect of the theory. We have seen that all three are based on the assumption that we must find a diversity in the cause, corresponding to each diversity in the effect. And it has been pointed out that this does not hold true even of admittedly physico-chemical systems.

Now, I do not claim that the bare word "mechanism," however hallowed by scientific usage, has any greater explanatory value than "entelechy." Indeed, I do not see why we should be called on to furnish a *mechanical* explanation, *sensu stricto*, of biological phenomena at all. Not all natural science is mechanics; some of it is chemistry. And I believe it is equally true that still another part is biology, a science quite distinct from either. But I think we can claim the possibility of a scientific explanation in the sense indicated by the foregoing antitheses, and it is with this in mind that I have grappled with the problem of organic "purposefulness."

(To be concluded)

¹³ It is not, however, necessarily "materialistic" in a metaphysical sense.